The Importance of Taxonomic Studies of the Fungi*

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The naming and classifying of living organisms has been going on for centuries. It has been well said that "a large part of our thinking about living things is bound up with some system of classification." Another writer has pointed out the fact that we depend much upon classification in our general experiences. "It is the innate propensity of active minds," he says, "to form species, *i.e.,* successively to make distinctions, to point out similarities, and then to assemble the things that are alike into their kinds. It applies to everything from chemical elements to college fraternities."

The recognition of the need of names for plants dates from the days of Pliny, the Roman naturalist, and Dioscorides, the Greek physician, in the first century of the Christian era. Plants could not be discussed without names. They could be named, however, without classification. They could be classified, also, without a conception of phylogeny. In other words, nomenclature deals with names which may or may not be arranged according to a system of classification; and classification deals with groups which may or may not indicate relationships. Many biologists, on the other hand, attempt to arrange groups on a basis of similarities, which they believe to be expressions of actual relationships. It is of particular interest today to note that the modern development of these aspects of botanical science has been made during the years since the founding of this Club. The first real progress in working out a universal system of nomenclature was made at an International Botanical Congress in Paris in 1867. A natural system of classification, although early recognized as desirable, has made its most progress since the theory of evolution provided a basis for phylogenetic interpretations. Darwin's Origin of Species, just a few years earlier, furnished the evolutionary concepts which soon became so significant in taxonomy.

Even a cursory examination of some of the early attempts to classify the fungi is sufficient to reveal that the results were most general in nature. Bauhin, in the days of the "herbals" purported to bring together all the plants known to him and to all those who preceded him (Pinax Theatri Botanici, 1623). The concept of the genus as a group of species had not then become definitely established. In the group which he called *Fungus* were included 81

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species which are now distributed to at least nine families. Tournefourt, in the latter part of the 17th century, made a considerable contribution to the genus concept. He recognized six genera of fungi and one of lichens. Dillenius and Vaillant added some genera and the latter published illustrations which were a real contribution to the study of the fungi. He maintained the genus *Fungus* in which were included most of the forms of the family Agaricaceae.

The foremost pre-Linnaean student of the fungi was Micheli. By the time of the publication of his "Nova plantera genera" in 1729 the microscope had become a working-aid and he made use of it. His work was excellent for the time. It included consideration of the genera of flowering plants, ferns, mosses, lichens, algae, and fungi. Both large and small forms of fungi were given consideration. He germinated and grew spores of the larger fungi and observed both mycelium and sporophores.

The early workers who studied the microfungi under the microscope rather naturally tried to interpret them in the light of their knowledge of the parts of flowering plants. In the case of the bread-molds the sporangia seemed like little fruiting pods containing seeds. By analogy rust spores were similarly interpreted although the situation there was not so easily demonstrated as with the molds. In 1807 DeCandolle, referring to the spores of *Uromyces* and *Uredo,* said that "with a microscope this powder seems composed of ovoid or globular spores . . . . filled with many small grains that are considered spores." He thought that a teliospore might contain at least 100 such "spores." This interpretation prevailed among such workers as Fries, Léveillé, and the Tulasne brothers, and persisted until the time of De Bary in the middle of the 19th century.

Linnaeus set himself the task of bringing together in his "Species Plantarum" (1753) all the known species of the plant world. He included the fungi in his class Cryptogamia but it cannot be said that he advanced the knowledge of them to any appreciable extent.

The first author to make a distinct advance in the classification of the fungi after the beginning of binomial nomenclature was Persoon. In a paper published in 1794 (Neuer Versuch einer Sytematischen Eintheilung der Schwämme, Romer's Neues Mag. Bot. **1**: 63-128) he recognized 77 genera of fungi, which he placed in two classes: Angiothecium and Gymnothecium. The three genera of rusts, which were included, were the first rust genera to be established after the solitary rust genus of Micheli 65 years before. Several authors of important works during the first quarter of the nineteenth century followed Persoon's classification in the main. Among these were Schumacher, Rebentish, Albertini and Schweinitz, De Candolle, and Brongniart. During the same period Link brought out a new classification which was accepted wholly or in part by Schlechtendal, S. F. Gray, and Wallroth.

During the middle of the nineteenth century great contributions to the knowledge of the larger fungi were made by Elias Fries. He had "not only a poor opinion of the parasitic fungi but an antiquated conception of their nature." In his third volume of "Systema Mycologicum" (1832) he used the name Hypodermii to include the rusts, smuts, and some other fungi and characterized them as having "No proper vegetative body; sporidia originating from the metamorphose of the cellular structure of living plants: an inferior kind of fungi." Nevertheless the work of Fries which extended over more than a half a century gave a great impetus to the study of fungi. His prestige was so great that there were many who accepted his leadership. Among these may be mentioned Endlicher, Léveillé, Corda, Rabenhorst, Strauss, Berkeley, and Cooke. Most of these authors made changes in the arrangement of the genera. Corda's extensive publication (Icones Fungorum) is notable not only for its contribution to the knowledge of the structure of the larger fungi but also for its advances regarding hundreds of the microfungi.

During the first three quarters of the nineteenth century new species were being recognized and named from all parts of the world. The descriptions appeared in journals, reports, and books many of which were not widely circulated. It is little wonder that investigators soon found it difficult to know whether or not a species under consideration was already described and named. It may be well said that this condition still exists. Thus it came about that species were named and renamed from several to many times. Little was known of the distribution of the fungi and workers in one region had no way of knowing of the probability of the existence elsewhere of the species which they were studying. Conceptions of the probable cosmopolitan distribution of the fungi were necessarily slow in developing. Many efforts were directed toward bringing together all species known to occur in certain regions or countries without attempts to determine their wider distribution. The flora-type of publication became common, especially in the European countries. Rabenhorst's "Kryptogamen Flora" of Germany, Austria, and Switzerland is a good example. Many other floras could be cited. These publications were valuable but they did not solve the problem for the workers who were located away from the European centers of mycological activity.

The assertion that many mycologists actually were deterred "from describing supposedly new species for fear of duplication" will doubtless not meet with credulity. An important step toward overcoming this situation was the plan for the "Sylloge Fungorum" inaugurated by Saccardo in 1882. The first volume appeared in that year. The effect was an immediate stimulation of systematic mycological activity. This great work developed into twenty-five volumes, the last appearing in 1931. During this period mycological journals were established in various countries and taxonomic work with the fungi went forward at a rapid rate.

Thus far we have given consideration chiefly to the describing, naming and classifying of the many and varied forms. The earlier workers naturally were concerned with these phases of study. It should not be concluded, however, that there were not some, even among the early workers, who were intrigued with the possibilities of studying the development and life-histories of the forms with which they worked. There were suggestions that relationships might exist between different forms which were found in close association. The impress left by De Bary on this phase of mycological work is well known. He began his work about the middle of the nineteenth century and the type of investigation which it stimulated has continued up to the present. He found time to work not only with fungi but also with algae, myxomycetes, bacteria, and higher plants. It is said that no less than 68 workers, afterwards distinguished in science, studied under him at Strassburg. According to Erwin F. Smith, "His work and that of his students put plant pathology on a new foundation, and he also, undoubtedly had much influence on human and animal pathology, since his very successful infection experiments with fungi on plants suggested many things to those who were trying to determine the cause of human and animal plagues." Yet we must agree that the primary interest of De Bary was in morphology rather than in pathology.

Using a good microscope and employing micro-chemical reagents De Bary made important advances in the knowledge of spores, infection, and mycelia. His cultural demonstration of heteroecism in *Puccinia graminis,* with proof that the aecidium on barberry was a stage in the life-cycle of wheat rust is well known. These results were announced in 1865. This work, and more which followed, ushered in a new phase of mycological endeavor. It is significant that he began these investigations not out of pure scientific interest, but in order to settle controversies between agriculturists and botanists regarding the relation between smuts and rusts and diseases. Agriculturists thought them to be the causes of disease while botanists were inclined to regard them as products of disease. De Bary had himself resisted the suggestion of a possible alternation of generations which required an alternation of hosts plants. When his experiments led to that conclusion, his naive statement that "one comes around, perhaps, in a way, to the ancient opinion according to which rusted wheat would be infected by the rust of barberry" is most interesting. His experiences should be heartening to many present-day investigators who are required to work on projects which are economic and agricultural in nature. Out of such problems may arise basic scientific discoveries as in the case of De Bary.

The next epoch in the study of the fungi after De Bary was ushered in by the study of the nucleus and its behavior. This gave a new direction to the study of fungi. As life-histories were important for taxonomic considerations so nuclear developments were eventually recognized as having a bearing on taxonomy. The application of cytological methods to the study of life-histories in the fungi began with the work of Dangeard in 1894 and was soon under way on a large scale. Other early workers in this field were Poirault, Sappin-Trouffy, Maire, Harper, Blackman, and Christman. It was soon evident that the nature of sexual reproduction in the fungi was of great value in determining relationships. We are indebted to such a host of investigators that it is impossible to mention them by name. Notable studies have been made in the Phycomycetes, Ascomycetes, Ustilaginales, Uredinales, and higher Basidiomycetes. In the last few years genetical studies have been made and highly important results are in the making.

Our account would not be complete if we did not make some reference to the possibility that the classification of the future may have a physiological basis. Much headway toward such a goal has been made by Mez and his associates. Many of you are familiar with the fact that Mez, using serological methods, has constructed a family tree of plants which corroborates in a remarkable manner the older tree based on morphological characters. Seifriz refers to this work in a recent book (The Physiology of Plants, 1938) with the remark, "It is of great significance to the field of evolution and phylogenetic relationship that a purely chemical basis of classification should so well support a purely anatomical one." Seifriz points out that the relationships between plants established thus far by serology hold well for families, not so well for genera, and not at all for species. He believes, however, that this is due to a lack of delicacy in technique. He is of the opinion species differences in proteins must also exist.

Our historical sketch which began with the early attempts to classify fungi led us rather inevitably to some consideration of morphological, cytological, genetical, and physiological studies. Certainly we must agree that knowledge gained in all these fields is essential for progress in taxonomy. E. A. Bessey in 1939 (A Textbook of Mycology) refers to the present-day activity of systematic mycologists and points out that, "Life histories are being studied in all groups, the sexual relations are being scrutinized from the lowest to the highest fungi and genetical studies are revealing results somewhat parallel, but on a vastly smaller scale as yet, to those attained by the study of *Zea mays* and *Drosophila*." "As never before," says Bessey, "is a knowledge of fungi themselves so necessary." Obviously right conceptions of fungi must be based upon many facts, and wrong conceptions can easily be the result of partial facts, and of ideas derived from other plants which may be inapplicable and misleading.

We have referred to the contribution which Darwin's theory of evolution made to biological classification. Phylogeny soon became the fundamental basis for classificatory endeavor. So far as the fungi are concerned we should not overlook the influence of the work of Hofmeister in 1851 on the bryophytes and pteridophytes. The recognition of an alternation of generations in these groups had its effect on studies of the algae and fungi.

Every student who has taken a course in general botany is familiar with the system of classification which places the algae and fungi together in the division Thallophyta. We have no thought of attempting to reach any conclusions about this broad question of the taxonomic disposition of the fungi. Whether the fungi are to be regarded as one of two subdivisions of the Thallophyta, the algae being the other, depends upon the origin of the fungi. We say this in spite of a recent assertion that the taxonomist "is not interested in the origin, but in the character of his plants." On the origin of the fungi, G. M. Smith, in his "Cryptogamic Botany," Vol. I, "Algae and Fungi" (1938) writes, "This is highly controversial and opinion is divided as to whether they arose from the protozoa or whether they had either a monophyletic or polyphyletic origin among the algae. If they arose from protozoa, they should be put in one or more divisions coordinate in rank with the various algal divisions; if they arose from the algae, they should be placed as classes of one or more of the algal divisions."

Smith reviews the algal and the protozoan theories of the origin of the fungi and concludes that "it seems more probable that the fungi evolved from protozoa rather than from algae." He bases his conclusion largely on metabolism and the type of flagellation in the Phycomycetes. There are some algal groups in which there occur chlorophyll-less forms which are so similar morphologically that they cannot be regarded as distinct from the green forms. It is pointed out that these saprophytic and parasitic algae accumulate reserve carbohydrates as starch just as do the green algae. In contrast the Phycomycetes are reported generally to accumulate carbohydrates as glycogen but never as starch. The zoospores and gametes of the green algae are never uniflagellate whereas the motile cells of certain Phycomycetes are regularly uniflagellate. It is admitted that the question of the origin of the Ascomycetes is a more difficult one. The similarity in the sex organs, and the structures developed subsequent to fertilization, in the Ascomycetes and in the red algae are striking and have caused many workers to assume a relationship between these groups. Smith argues that these distinctive reproductive structures may have evolved along independent phyletic lines. He thinks the Ascomycetes had their origin in the Phycomycetes and that the Basidiomycetes arose by modification from the Ascomycetes. In his classification he rejects the Thallophyta as a division of the plant kingdom and in its place substitutes nine divisions, of which the Myxothallophyta, or slime molds, constitute one and the

Eumycetae, or true fungi, constitute another. The other seven divisions include the algae. "Abandonment of the Algae as a subdivision of the plant kingdom," says Smith, "does not mean that the word *alga* must be abandoned." He believes that we can still use the term alga for designating simple green plants that have an independent mode of nutrition. We might add that we will likewise continue to use the term *fungus* although attempts to define it lead to difficulties.

Bessey in his "Textbook of Mycology" has attempted a definition of the term fungi that would not commit the definer to any system of classification. We quote: "Fungi are chlorophyll-less thallophytic organisms typically consisting of coenocytic or cellular filaments, but including also encysted or amoeboid one-celled organisms which reproduce by some type of motile or non-motile spore; excluding the Bacteria and such chlorophyll-less organisms, which, by their structure, are with definiteness assignable to recognized orders of algae." Bessey is of the opinion that the Mycetozoa are not related to the fungi; are not, indeed, plants. There are those who believe that the fungi should not be regarded as belonging to the Plant Kingdom. Herbert F. Copeland in a comparatively recent paper (Quarterly Review of Biology, December, 1938) has presented evidence and argument "to the effect that organisms can be arranged, naturally, and more conveniently than in the past, in four Kingdoms as follows":

Kingdom 1. Monera (Bacteria and Blue-green Algae)
Kingdom 2. Protista (Protozoa, Diatoms, Red and Brown Algae, Slimemolds, and Fungi)
Kingdom 3. Plantae (Green Algae, Liverworts and Mosses, Ferns and Allies, Seed plants)
Kingdom. 4. Animalia (Metazoa)

To those who have been accustomed to thinking that all living organisms must be either plants or animals the recognition of two new groups as Kingdoms may seem revolutionary. It is true, however, that the line between lower plants and lower animals has always been a difficult one to draw. It must be admitted that nomenclatorially there are difficulties in placing together in the Kingdom Protista organisms which have been previously in two different Kingdoms. The original proposal for a Kingdom to be called Protista was made by Haeckel in his "Generelle Morphologie" in 1866. He also established the group Monera but included it in Protista. According to Copeland other authors have expressed the opinion that the Monera should be treated as a separate Kingdom.

The comments presented here relative to the origin of the fungi form a very inadequate picture of the discussions and arguments that exist in the writings of many investigators. We have wished merely to call attention to the fact that there is no general agreement as to whether the fungi are monophyletic or polyphletic in origin or whether they have descended from the algae or from the protozoa. The algal theory appears to have been advocated by A. Braun in 1847, and was accepted by Cohn (1854), Pringsheim (1858), and Sachs (1874). De Bary in 1881 objected to the method of intercalating the fungi among the algae saying it led to an orderly arrangement of species but not to a natural system. The suggestion that the fungi arose from the protozoa is credited to Cornu (1872), and was developed by Gobi (1885) and Dangeard (1886). Atkinson (1907) was in favor of deriving the lower fungi from ancestral unicellular organisms, but was uncertain whether they were colorless or chlorophyll bearing. He was, however, certain that their origin was monophyletic. The algal origin of fungi was supported by Strasburger and C. E. Bessey. Gäuman (1925) presented the view that all true fungi were derived from the green algae in monophyletic line; he believes the lower Chytridiales (his class Archimycetes) along with the Myxomycetes may have arisen from the colorless Flagellatae. He does not regard either of these groups as fungi. Martin (Bot. Gaz. **93**: 421-435, 1932) has "suggested that the fungi be regarded as a phylum which has not definitely developed into either plants or animals, but may be grouped with the former as a matter of convenience, and in accordance with custom." He rejects the assumption that all living organisms are descended from a single primitive cell and points out that the assumption that life may have originated more than once and in different forms is more in accord with what we know of living organisms.

Clements and Shear (Genera of Fungi, 1931) enunciate a basic principle: "that the fungi do not constitute a natural group, and that all the phyletic lines lead sooner or later to holophytic origins." It should be noted that although they say they are not dealing with a *natural* group yet they claim to have approximated a *natural* system in several respects in their book. They believe that there is but one natural system and they maintain that any approach to it must be the result of the work of many minds. After their admonition that it is more or less inexact, even though convenient, to connect the name of an individual to any particular arrangement, one wonders whether he should not tear up his manuscript and begin anew. Clements and Shear do not agree that cytology can be the final arbiter on questions of origin and relationship among the fungi. They make a plea for experimentation "on the largest and broadest scale possible, in both field and laboratory."

This review which is concerned with the taxonomy of the fungi must provide reference to the specialists who publish papers or monographs on certain groups. Sometimes such authors are called experts. I like the way one writer who says he is no expert disposes of this matter. He says, "The standard taxonomic revision is the work of an expert in the group concerned; it cites all the present literature; it is received with respectful interest (never with complete acquiescence) by the author's fellow experts in the same group, and is more or less annoying to others who have to take it into account, as requiring revision of familiar ideas of the limits of groups and the application of names." The parenthetical phrase is not mine; it is in the original.

As with other groups of living organisms the fungi have had their devotees. Crowds of them have advanced to the expert stage. It is impossible to name them or to evaluate their contributions. They must be treated generically, as it were. The writer has thought it worth while to try to present some of the problems which such workers encounter. By this is meant not so much the problems inherent in taxonomic studies but rather the wider limitations which often operate to check individual progress and to break the continuity of advances for which a groundwork may have been well established. The difficulties which are to be discussed are not necessarily peculiar to systematic mycology. Taxonomic work in general as well as in mycology, has a checkered history. Its advances through the centuries have been piecemeal. Perhaps it will always be thus, and deploring the fact may not only be in vain but may not be fitting.

It seems likely that we must depend largely upon institutions to furnish the support for taxonomic mycology. Of course there have been numerous individuals who have done their work chiefly or wholly without institutional support. In this country we have only to think of such men as L. D. von Schweinitz, J. B. Ellis, C. E. Fairman, J. J. Davis, and Elam Bartholomew, to realize the debt we owe to individuals, and great credit is due them.

Even where universities, colleges, or other institutions or governmental agencies are involved it is still true that the ambition, industry, and perseverance of individuals are largely responsible for the advances that have been made. In these later days we have been hearing a good deal about institutional research. So far as taxonomic work with the fungi is concerned we delieve that an analysis would show that research in this line is mostly due to individual prosecution rather than to institutional initiation. It may happen that an institution will make an effort to continue the type of research that has been inaugurated and successfully carried on by one of its staff members and will then refer to the program as an institutional program. More often it happens that a real leader appears and develops successfully a line of work which is supported (more or less) during his years of activity but which is dropped by the institution afterwards. Such instances indicate the correctness of the conclusion that there is often no such thing as an institutional program. There are, of course, exceptions but we feel safe in saying that the exceptions prove the rule rather than make it. We have inserted the parenthetical phrase—more or less—because we are sure that institutional support even when forthcoming during the height of the program is often more apparent than real. Certainly it is true that many of our productive mycologists have had to earn their "bread and butter" with teaching and routine duties and have had left only a small percentage of their time and efforts for the kind of work which they were so well qualified to pursue.

Someone may well ask why these difficulties are raised in connection with taxonomic research when they exist in so many lines of research activity. There are several reasons for doing so. The source materials for taxonomic research are in large part not commercial commodities. They consist of rare books, separates, indexes, illustrations and specimens which are accumulated only with time, patience, correspondence, and exploration. When such collections have finally been put together in an institution they should be used by more than one generation of workers in that institution. Or if that is not possible some method should be worked out by which they become available to succeeding investigators in other institutions. There are now in existence some collections of microfungi where spore measurements and drawings accompany literally hundreds of specimens. Such aids are indispensable for taxonomic studies and when available not only save the time necessary to duplicate them elsewhere but help to prevent errors and misconceptions. There are also herbaria of fleshy fungi where great accumulations of photographs, drawings, and notes make them of the utmost importance to other workers. This is not a plea for the centralization of mycological taxonomy. It is rather to call attention to the fact that enormous resources are frequently accumulated and then not used nor made available for use. Since our modern concepts fix the application of names by types rather than by descriptions it is a fair question whether type specimens should ever be personal or institutional property. The difficulties may seem insurmountable but this may not be the case. Surely we will make no progress until the workers themselves reach a keener appreciation of the situation.

There are other factors which bear on the progress of taxonomic work with the fungi. Even though a staff member may have the ability and enthusiasm to carry on work of this sort it may be, as previously indicated, difficult for him to obtain the full cooperation of his institution. Projects which have more evident economic aspects have always elicited more favor with administrative officials in our agricultural institutions. This is true in spite of the obvious relation of taxonomic studies of the fungi to many phases of plant pathology. It is easy to comprehend why this attitude prevailed in the early days of the agricultural experiment stations but it is not so easy to see why the value of fundamental work of this sort should not eventually come to be recognized more generally. In very recent times approval of agricultural projects depends upon evidence that results are likely to be of direct benefit to farmers. And again, even though there may be institutional approval so far as the time of the worker is concerned, it is often difficult to secure the maintenance support which is essential. For a project requiring special apparatus, machinery, glassware, and chemicals, it is usually not difficult to secure funds. But to secure funds for the purchase of specimens, photographs, particular books, separates, periodicals, indexes, and exploration it may be difficult or well-nigh impossible. It is generally conceded that a research worker is not expected to get along with the equipment and supplies which are in general stock but is entitled to special expenditures for his project. Not so with library facilities. He may be expected to get along with what the institutional library provides. He may of course compete for more than his share of the general library funds but this is not always satisfactory even if partially successful. The use of research funds for special library facilities is much less common than for special material equipment. The problem of publication is a closely related one. Monographic treatises are often expensive to publish and the demand for them may be slight and slow. The fact that publication is difficult tends to discourage this type of work.

A few weeks ago I received a letter from a former associate in which he said, "I notice, with much interest, in the last issue of Science, that you are to have a part in the 'Symposium on Taxonomy,' June 23, in connection with the Seventy-fifth Anniversary Celebration of the Torrey Botanical Club . . . . I assume that you will *speak for the fungi.*" Of course. Whether I have said, or still can say, anything which he would have me say is another matter. I assume that he expected me to make some reference to the problem of nomenclature and it seems impossible to close this discussion without bringing up this vexatious topic.

I propose to make comments of a general nature and to confine them to two aspects of the nomenclatorial situation: (1) on getting rules, and (2) on getting them into effect.

It is generally conceded that "Natural history can make no progress without a regular system of nomenclature, which is *recognized* and *used* by the great majority of naturalists in all countries." This is a quotation of the first article of the International Rules of Botanical Nomenclature; the italics are mine. The necessity of establishing international rules to govern the application of names of plants has been recognized by botanists for many years. But it is easier to recognize the problem than to solve it. The world well knows the difficulties of securing unanimity of action on any matters calling for international consideration.

One of the chief difficulties is to get together a group, the personnel of which is truly representative of the science and at the same time really international in standing. Institutions and governments have been willing to designate individuals as representatives to botanical congresses but for the most part they have been unwilling, or thought it unwise, to contribute toward the expense of attendance. The final assembly has been made up, therefore, not necessarily of those best qualified but of those individuals who have been willing to finance a trip in order to take part in the proceedings. The departments of our national government sometimes send "official delegates" to international congresses but they usually place restrictions on the activities of such delegates. I hope I am giving away no secret when I say that an employee of our federal government told me when we were in attendance at an International Botanical Congress that he was instructed before leaving this country that he might take part in the discussions but was not allowed to vote on the questions coming before the section on nomenclature. The conclusion seems to be justified that the advancement of this phase of natural history, of the greatest importance to mankind, has been too dependent upon voluntary contributions of the workers themselves.

It is also generally conceded that rules of nomenclature should not be arbitrary and that they cannot be imposed by authority—at least not by the authority of the makers of the rules. As an alternative the framers of the rules say, "They must be simple and founded on considerations clear and forcible enough for everyone to comprehend and be disposed to accept." Such a statement was made in the Rules as published in 1912 which were adopted in 1905 (Vienna) and supplemented in 1910 (Brussels). Perhaps rules of nomenclature are like a plant which grows slowly and requires a period of development before it comes to maturity. I do not know how many people did not comprehend the International Rules of Vienna and Brussels but I do know that in the following years many were disposed *not* to accept. There were individuals and groups of individuals who deplored the fact that certain fundamental principles of a basic nature in which they believed were not incorporated. They felt that once they accepted a code without these principles the chances for amendment would not be good. I have in mind chiefly the "type-concept" which was not a part of the original code. Reference to a more or less minor feature may serve to illustrate difficulties regarding adoption. The Vienna code provided that "On and after January 1, 1908, the publication of names of new groups of recent plants will be valid only when they are accompanied by a Latin diagnosis." Again I do not know how many names have since been published which are invalid, but I do recall taking part in a business session of a certain mycological society, at least 25 years after the Latin deadline, when the matter before the house was whether that rule should be enforced in its official journal.

It seems fair to say that cordial agreement was reached at the Cambridge Congress in 1930 on most of the disputed nomenclatorial problems and that the disposition to accept International rules was improved thereafter. Not long ago I was criticized by a colleague for such a conservative statement. He wanted me to say that these rules are, and have been for some time, actually in effect. Again it may be time which settles many problems. At any rate, it was in 1940 that the Secretary of the United States Department of Agriculture formally approved a recommendation of the Department Committee on Plant Names "to put the Department, botanically speaking, under the International Rules of Nomenclature." To me it is interesting that it took ten years for this department to come to an action making these rules official for "publications, reports, and correspondence involving scientific plant names." Perhaps one might be pardoned for calling attention to the anomaly of an agency finally finding it expedient to subscribe to the acts of an organization which it failed officially to aid. It is also interesting to note that two years after the official order they are still going through an adjustment period in getting nomenclatorial usage realigned according to International rules. When it becomes necessary to drop the name *Ustilago hordei* which, according to old usage, has been applied to the *covered* smut of barley and to take up the same name, according to International Rules, for *loose* smut of the same host it is little wonder that the workers talk about confusion. Personally, I believe that the confusion will be only temporary and that the advantage of getting on a world-usage basis will more than outweigh the disadvantages. It is desirable to avoid changes in names as far as possible, but changes cannot be entirely avoided if the rules of nomenclature are to put in order the old names as well as to be a guide for the creation of new names. There are those who believe that the procedure embodied in the present system of nomenclature leaves too much to expediency and personal preference and do not rest sufficiently upon foundamental principles. It has been pointed out that "there is no guarantee—if, indeed, there is any hope—that the system which may be adopted today will be accepted by the next generation." No, there is no guarantee that anything man devises will continue—not even democracy. We must not, however, look upon this or any other problem in such a futile manner. There are difficulties, to be sure, but they are not insurmountable. We are told in the Torrey Botanical Club Announcement and Field Schedule for 1942, "It is understood that there will be no mutilation of species at this session." That being the case, this seems to be the proper place to bring this discussion to an end.

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